College of Letters and Science.

FACULTY.

Rev. W. H. H. ADAMS, D. D., President,
Professor of Ethics and Metaphysics.
H. C. DeMOTTE, Ph. D., Vice-President,
Stamper Professor of Mathematics and Astronomy.
SUE M. D. FRY, Ph. D.,
Professor of Belles Lettres.
CHARLES M. MOSS, M. A., Secretary,
Kimber Professor of Greek.
Rev. ALBERT J. NAST, M. A.,
Professor of Latin and German.
LUCIEN M. UNDERWOOD, Ph. D.,
Isaac Funk Professor of Geology and Botany
GEORGE B. HARRISON,
Curator of Museum.
J. M. GILLAN,
Instructor in Elocution.

THE

ILLINOIS WESLEYAN UNIVERSITY,

Bloomington, Ill. Feb. 2 1882.

Prof. Watson,

Dear Sir:-

I have recently purchased all of the late Col F. Austin's manuscript on the Hepaticæ, and find among his writings a "Synopsis of the Californian Genera" in his own handwriting (pencil) and some 6 legal-cap pages of descriptions of genera and species of Californian Marchantiaceæ in what I judge to be your handwriting. I would be pleased to know if this was all Mr. A. had prepared for his part of the "Botany of California"

In the preface to the second volume of that work you speak of the manuscript left by Mr. A. as being "incomplete."

As I expect to devote my leisure time to the Hepaticæ I would be pleased to know if any thing more was written than I now hold as indicated above.

Respectfully,

Lucien M Underwood.

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THE

ILLINOIS WESLEYAN UNIVERSITY,

Bloomington, Ill. June 7th. 188-

Mr. Watson;-

Dear Sir:-

Your letter respecting Mr. Austin's Manuscript was received some time since and I desire to thank you not only for the information but also for your good wishes regarding my own work in that Department. I desire now to ask another favor hoping I am not troubling you beyond endurance.

In my forthcoming "Our Native Ferns and their Allies," I desire to include all the Pteridophyta and I am as yet unable to find the specific description of Selaginella rupestris of A. Braun. It is not to be found in Spring's Monographia Lycopodiacearum which I have. If you could get this for me without too much trouble you would greatly oblige

Yours truly

Lucien M. Underwood

Syracuse. N.Y.
6 Feb. 1886.

My dear Doctor Gray

You have placed me under great obligations to you for the treasures contained in the package sent me this week. I have long been trying to get together a set of your publications and had only indifferently succeeded. I had all the quartos but the "Chloris" and that I had given up all hope of picking up. It took four years to find "Plantae Fendlerianae" For this and many past kindnesses I can only say, thank you, but this with all the gratitude these words can express.

Hepatics come in very slowly but I will remember to supply the Cambridge Herbarium whenever possible. Yours L. M. Underwood

Syracuse University
Syracuse, N.Y.
6 May 1887

Prof. Sereno Watson

Dear Sir:-

The hepatics and drawings are at hand I will care for them and return them in due time. I have in preparation a paper on the Californian Hepatics for the Cal. Acad. of Science. I was expecting to illustrate some of them forms that are as yet undescribed but was not aware of the existence of these sketches of Gottsche which you have sent with the specimens.

Would there be any impropriety either toward Gottsche or others in my making use of some of his admirable sketches in preparing a plate of illustrations (of course giving him credit as well as yourself)? If this would in any way be out of harmony with

botanical courtesy I should be grateful to be informed. If I were to do this it would simply have the effect to make a better plate.

I am not even aware as to whether Gottsche is living. I once addressed him at his home but received no reply. I have never seen notice of his death. Can you inform me?

Truly yours

Lucien M. Underwood.

Syracuse, N.Y.
23 Dec. 1887

My dear Prof. Watson

I am preparing the copy for a new edition of my work on Ferns to be published by Holt and to use the language of the press the d—— is after me for copy. I find in reviewing Selaginella that Baker in his recent work has added some to our species and as his conclusions in other places differ much from mine I am anxious before publishing to go over a larger collection of specimens than my own herbarium offers and see particularly Wright's and Drummond's S. W. specimens. Having received so many favors from you and Dr. Gray I feel that it is almost an imposition to ask another and yet I cannot

at present spare the time or money to make a trip to Cambridge.

If you are willing to let me take the N.A. specimens of Selaginella for a week or ten days it will be an invaluable aid to me in my work.

Have you Suksdorf's Exsertes No. 836 which Baker describes as new (I. Suksdorfii) with trilobed trunk or rootstock? I have sent to Suksdorf for this but it takes an unconscionable time to reach him and he may be away from home too.

I wrote to Dr. Gottsche as you suggested and have his consent to use the materials on California hepatics. He also sent me specimens and diagnosis of Grimaldia Californica nov. As soon as I am over my hurry on the Fern book I shall make drawings for a plate

and publish in the Botanical Gazette after which I will return Specimens and ~~[illegible]~~ things you so generously loaned. I am anxiously hoping for Dr. Gray's recovery sufficient to enable him to complete the Polypetalae — his European work at least.

Yours very Truly
Lucien M. Underwood

Syracuse N.Y.
9 Jan 1888

My dear Professor Watson

I returned the Selaginellas on Friday last and wish to return you thanks for the use of them. The Mexican and West India species are especially interesting but I was surprised to find so few of our N.A. species wanting. I will bear you in mind as specimens come in.

S. apus
S. lepidophylla
S. selaginoides
S. Douglasii
? S. Ludoviciana A.Br. ~~Baker~~
? S filifera A.Br. said to be collected in Texas by Wright are not in your coll. I will endeavor to finish work on the Hepaticae

this week if possible and return them. You will remember that at your suggestion I wrote Dr. Pottache asking permission to publish. After his reply came I was interrupted by my Southern trip which has left me far much behind that I cannot finish work on them before.

Yours truly

Lucien W. Underwood

Syracuse, N.Y.
21 Jan. 1888.

My dear Dr. Watson,

I am under renewed obligation to you for the further loan of Selaginella. I think Drummonds plant is the true S. ludoviciana A.Br. and have referred to it also Curtis' No. 3799 yet the forms I think will run into each other when we come to have extensive suites of specimens for study.

Pringle's No. 271 in your collection is S. Pringlei Baker (Cf. Fern Allies p. 89 no. 209*) I enclose a bit of true S. cuspidata which you will see is very diffrent. (I see S. cuspidata is reported by Eaton as occuring in Your collections in his report on Mexican Ferns in your "Contributions" so I presume a spec-

imen of it was in the set I had before).
I neglected to take the precise locality of S. tortipila from N.C. as ~~taken~~ given on the label. May I further trouble you to send this on enclosed card.
I have some of the S.C. material myself sent by J. Donnell Smith. Baker makes tortipila a variety of S. rupestris I think justly.

Yours gratefully
L. M. Underwood.

P.S.
I return the material by Express to-day.

Negro Bluff — Gray
Table Rock, S.C.
Caesars head " Smith
Saboola Mt., Macon Co., N.C. Smith

Syracuse, N.Y.
7 June 1888.

Dr. Watson

Dear Sir:-

I rejoice to know that you are preparing a new edition of the "Manual" and will gladly do what I can to aid in the matter. I am also pleased to know that those plates of Sullivant's are still in existence. Quite a number of changes of name will have to be made on them, however, but I suppose this can be easily done as it was, I think, in the case of the plates of the Mosses.

I leave for California on 15 June to be gone three months but will have the material ready for you at the appointed time.

I am also preparing for a revision of the California species and a revision of one or two of the larger genera. My material has largely augmented since a year ago. I have even been unable to have a set of Austin's Hepaticae until I purchased Dr. Ravenel's set last October. I hope to do considerable collecting this coming season in California, altho' I visit it at an unfavorable time.

Is Conocephalus preoccupied as a generic name? European authorities are about evenly divided between this name and Fegatella. I see no reason for retaining the latter unless the first is already in use and of this I have no means at hand to determine.

3/

✓

A few plants not mentioned in the former editions of the "Manual" are common here and in places becoming more so.

Carum Carui is a weed in many parts of Central New York.

Tragopogon porrifolius is becoming quite frequent as an escape from cultivation and I remember seeing the same in Central Illinois two years ago.

Ribes lacustre is quite common at Green Cove 1 m. east of Jamesville, Onondaga Co., N.Y. though this may be too common a plant to specify localities for.

Yours very Truly

Lucien M. Underwood.

Syracuse, N.Y.
26 Sept. 1888.

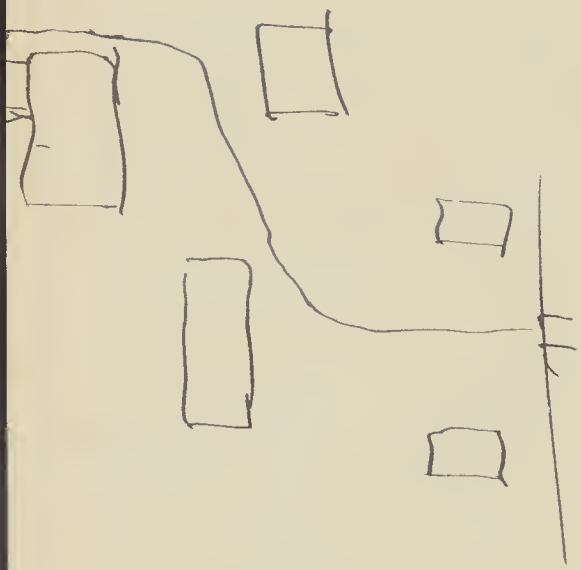
My dear Dr. Watson,

I am back from California and have already done considrable preliminary work on my Mss. for the Hepatics. I fear I shall have to trouble you in regard to several points later. A few I have met with already.

1. A little more on the Conocephalus-Fegatella matter.

Dumortier (Hep. Europ. p. 154) says "Conocephalus Neck. Elem. Bot. III, 344 (1790)." If the genus is here well characterized or even recognizably characterized and is older than the Urticaceous genus Conocephalus it would seem that it ought to stand. I have never seen Necker's work. Do you have it?

2. Is there in Sullivant's collection a plant



2/named either "Rebouliа Sullivantii" or "R. hemisphaerica, var. gracilis" I find a species described under the former name by Lehmann. Pug. Pl. X, 24 (1857). If it is these I wish I could see it.

3. Riccardia was established by Gray Brit. Pl. I, 683 (1821); Aneura by Dumortier in Comm. Bot. 115 (1822).

Lindberg (Hep. in Hibern Lectae) and Carrington and Pearson. Hep. Britann. Exsic. adopt Riccardia. Spruce, however, whose authority on hepatics is probably unexcelled uses Aneura Dm. and gives as a synonym (Hep. Amazon p. 548) "Riccardia Gray pro parte". On general principles is Spruce correct? Or as he includes in Aneura l.c. Pseudoneura Gottsche, ought he not to have used "Riccardia Gray, Spruce emend."

Spruce uses many of Gray's genera e.g. Scalia for Gymnomitrium; Nardia for Alicularia; Pallavicinia for Steetzia; Herbertа for Sendtnera; Under Scapania however he makes no mention of Martinellia Evoluta Lindberg has adopted for Scapania! Helvin

3/ in this, however, Lindberg has no following.

4. How about species found only in Canada Labrador or farther North and which do not come within the limits of the U.S.? Do you want any of these included or shall I exclude all even though they are known from near the U.S. boundary E.g. from shores of L. Superior.?

When I come to Lejeunia I shall have some more ~~f~~ questions in regard to Taylor's localities which I find to be at fault according to Spruce. Frullania I shall leave until the last as I expect to revise all the N.A. species. I have specimens gathered by myself from 80 localities ranging from Macon, Ga. to Washington Territory.

I suppose there will be no difficulty in changing generic names on Sullivants plates. How soon would you need to know what these changes are?

Are copies of Sullivants Icones Muscorum and Supplement on sale anywhere?

Yours truly Lucien M. Underwood.

Orth. 57/44

Conocephalus of Redtenbacher, as determined at least by his figures, is the same as Xiphidium, & should stand for our phidium & cause cause.

"Rhodonia subtilis" = specimen auct.

Riccardius, Gray = see Carruthers,

Journ. Linn. Soc. 1865. should apparently take the place of Amblycorypha.

Martinellius. Lindberg is right. Keep Rehnia for Rehn's type section (Rehniella), Martinellius for the rest of Gray's section A (i.e. Scaphuricia), and Platycleis for the remaining sections.

Do not include Canadian species unless positively known to be found there, as on Mr. Rhoads' list of L. Sups. in N.Y. Museum.

No hard & fast Thamnotrizon,

Subf. Le. Stenopel. = H 22,00

XX

20 Sept 1884

Syracuse, N.Y.
8 Nov. 1888.

Mrs. Asa Gray
Cambridge, Mass.

Madam: Please accept my sincere thanks for the valuable "List of the Writings of Dr. Asa Gray" which you have had the kindness to send me. It is a valuable and timely contribution to the Bibliography of American Botany.

Yours very truly
Lucien M. Underwood.

Syracuse, N.Y.
12 Dec. 1888.

My dear Dr. Watson.

I have delayed answering your letter hoping to get Gray's Nat. Arr. of British Plants which I had sent for and now have.

I have thought that for a general principle it is not advisable to make changes where there is uncertainty and when a reverse change may have to be again made.

Leaving Germans out of the question I consider Dr. Richard Spruce as the highest living authority on Hepatics and next to him is Dr. Lindberg. Where these have both agreed as touching Synonymy etc. I have thought it safe to follow them providing they satisfactorily prove their case. When they differ I think it safer to make haste slowly and

do nothing which subsequent writers may have to undo. Am I right in this?

Thus far in changes they agree:-

Pallavicinia Gray for Steetzia

Porella Dill. for Madotheca

Herberta Gray for Scudderia

Spruce retains Aneura and Scapania

while Lindberg substitutes Riccardia and Martinellia

Spruce and Lindberg moreover recognize Cesia Gray. but Carrington (Brit. Hepaticae p. 4. et seq.) uses Gymnomitrium claiming that Caesia R. Br. Prodr. Fl. N. Holl. I, 277 (1810) (Liliaceae) precludes the use of Cesia Gray. How is this? Is Caesia valid and too near Cesia?

Stephani, Limpricht and, I guess, Germans generally do not recognize Gray's Genera.

In regard to the Conocephalus matter I see no reason why Hooker's description does not establish his genus. I have little doubt, but that it will eventually be referred to Hepatica Mich. I see no objection to this inasmuch as we recog-

3/

nize some of Micheli's genera e.g. Targionia and Sphaerocarpus. Pearson, Lindberg and others favor the use of Hepatica I do not know that Spruce has expressed himself on the subject or mentions the genus in any of his recent papers. I should be glad for your opinion on this

I enclose a list of plants I would like to see if I could. In regard to Taylor's Hepaticae I suspect that he has made several errors in attributing N.A. localities to E.A. plants. So far as I know they have not been found in the U.S.! If there can be found nothing to corroborate their existence had they not better be appended as doubtful species under the genus?

I would like also to borrow all the material you have in the genus Fullania as I wish to work this genus up as fully as I can. I have a very large supply of material collected by myself and shall have considerably more from other sources. I return the Rebouldia from Sullivant's Collection

Yours truly
L. M. Underwood

Scapania brevifolia Tayl.
Metzgeria pubescens from Manual Region
Frullania Tamarisci " " " = F. Eboracensis
F. fragilifolia " " "
F. inflata (St) v. monoica " " "
Lejeunea calyculata Tayl.
L. Cyclostipa Tayl
L. Polyphylla Tayl.
L. ✓ Lesueurica Tayl.
L. ✓ longiflora Tayl.
Madotheca Thujæ " " "
Chiloscyphus Drummondii Tayl.
Pleuranthe olivacea Tayl.
Jungermannia Gillmani Aust.
J. Walliana. Aust
J. Wallrothiana
Odontoschisma Macounii (Aust)
Jungermania Sullivantiæ

Of the above I have no authentic specimens from the Manual region. Of most I have no specimens whatever. If you have any of them ad the herbarium I would be pleased to see them.

Scott & Underwood — Feby 6/89

Fernblania — 71 sheets of Taylor Herb^m

48 " of main Herb^m

Scaphonia frenciflora — Tayl. Herb.

Chiloscyphus Drummondii " "

Lejeunia calycularis " "

" platyphylla " "

" cyclodisca " "

" tisbudiana " " + Gott. Herb.

Pleurozwtha obvoluta " " + Gott. Herb.

Lejeunia longiflora — Gott. Herb.

12 Dec 1888

Syracuse, N.Y.
2 Feb. 1889

My dear Dr. Watson,

A few names are doubtful as to origin and I have not Micheli's Nov. Plant. Genera to verify. I have the impression that Gray took most of his hepatic names from the list of Micheli's patrons. I think these are all I lack –

Herberta Wm. Herbert
Bazzania
Kantia
Nardia
Pallavicinia

I was unexpectedly called to California the day after Christmas and lost three weeks on account of that trip which however was a very enjoyable one

Yours truly L. M. Underwood

I send hepaticæ Americanæ Decads III and IV for Herbarium

Syracuse N.Y.
15 Feb 1889.

My dear Dr. Watson.

The package of Fullania came to hand in good condition and I am glad to have the help received from it. I will probably be through with it next week, and will return it at once.

There are nine genera of Hepaticæ that will have no illustration. Of these seven are represented by only a single species. Jungermania and Nardia are more numerous. I would hardly recommend the preparation of a new plate under the circumstances. Many species in genera now illustrated in the three plates need it fully as much as some of the genera which are

not represented. May I offer one suggestion with reference to the "Manual" at large. A great many use the "manual" without the "Lessons" or at least bound in separate covers. It would be the greatest convenience to have a glossary bound up with the Manual itself when used in this way. It is even more important with the "Manual" alone than with the "Lessons" alone.

I enclose a problem in synonymy which I would like your views upon as I have met several. There is a difference of usage. To make you the least trouble I enclose the problem on a card addressed to me.

Yours very Truly
Lucien M. Underwood

Syracuse, N.Y.
4 March 1889

My dear Dr. Watson

The MSS. on the Hepaticæ is ready and I will send it when and where you direct. I will return the Frullanias in a day or two. I have one more question, viz: the origin of the generic name Duvalia which I take to be a personal name. I enclose a list of changes necessary on the plates. I have made no changes here or in the text which are not approved by Lindberg, Spruce, Carrington, Pearson and other eminent living hepaticologists.

Yours very truly
L. M. Underwood

Changes in Sullivants' Plates of Hepaticæ

Tab. XX	Reboulia	change to	Astercella
	Plagiochasma	" "	Aitonia
	Fegatella	" "	Conocephalus
	Steetzia	" "	Pallavicinia
Tab. XXI.	Pleurantha	" "	Harpanthus
"	Jungermannia	" "	Cephalozia
	Sarcoscyphus	" "	Marsupella
Tab. XXII	Sphagnoecetis	" "	Odontoschisma
	Madotheca	" "	Porella
	Mastigobryum	" "	Bazzania
	Sendtnera	" "	Herberta
	Calypogeia	" "	Kantia

Syracuse, N.Y.
9 March 1889.

My dear Dr. Watson

I have carefully packed the Emelania's and my MSS and send by American Express to-day.

Since Dracalia must go, Grimaldia will have to take its place and I have changed the MSS. accordingly. Hoping you will find everything satisfactory, and thanking you for your great kindness in sending the Emelania and in many other ways, I am

Yours sincerely
L. M. Underwood.

Syracuse, N.Y.
8 Apr. 1889

My dear Dr. Watson

In regard to cutting down the V.S. I think it can be done in the small and especially the monotypic genera. In a few of the larger genera especially Cephalozia, Jungermania & Frullania, Lophocolea and Lepidozia it is desirable to end as little as possible. The "Systematic Arrangement of the Jungermaniaceae" might be omitted which would save a few pages.

I did not think of the necessity of bringing the keys in harmony with the Manual when I arranged them on the A B C plan. I will revise that if you would like though personally I have a very strong prejudice in their favor on the grounds of simplicity, ease of reference and economy of space.

When is it expected that the new edition will be out? I have a class ready for it

tomorrow but shall wait for the new edition.

I enclose a fragment of a plant sent me as a "Minnesota wild flower" that I can make nothing of. It seems to have the tubular ~~corolla~~ petals of the Helleborus section of the family combined with the habit of a Ranunculus.

Yours truly

L. M. Underwood

[illegible]

Dear Sir:

I am preparing an article on "Bird" and [illegible] "Game Sections" and, [illegible] [illegible] can [illegible] furnish. Will you kindly [illegible] [illegible] and return to me as soon as possible? If not able to give exact [illegible] approximate numbers, give as careful an estimate as possible; in such case state that the number is an estimated one.

Any suggestions you may offer in this matter, or any reference to private collections that you think likely to be worked would be gratefully received.

Yours very truly,

L. M. Underwood

Dr. Serno Watson
Cambridge, Mass.

(over)

Does the Sullivant Collection have any limitation as to its being kept distinct? If so can you estimate that separately?

In regards to the MSS of the hepatics I will say that I have cut down the copy as much as definiteness and accuracy will permit and have corrected the form of the keys etc. to conform with the rest of the Manual. The MS is at your disposal when needed.

Yours truly
L. M. Underwood

P.S. Do you ever have exotic ferns in duplicate for sale or exchange?

Syracuse N.Y.
23 Sept. 1889.

My dear Dr. Watson

While I was at the White Mts. you sent for the Mss. for the Manual and my sister who had charge of my mail sent on the copy. I find Botrychia julacea (Jung. julacea of my catalogue) in our White Mts. material reported hitherto only from Greenland and doubtfully from Cal. This ought to be included. Shall I include it in the proof? I suppose the proof will be sent to me in any case.

I think I wrote you in regard to the change of names on the plates of the Hepaticæ; if I did not will will you please let me know it.

Yours in haste
Lucien M. Underwood.

Syracuse, N.Y.
20 Oct. 1889.

My dear Dr. Watson.

In regard to Frullania Virginica I was misled by Gottsche's unwarranted appropriation of the name in Synopsis Hepaticarum. I have seen Lehmann's Pugillus VIII (two copies) but had not noted the error. I have in my library only Pugillus X. It should read F. Virginica Lehm.

Will you please insert the enclosed Lejeunea lucens in place of L. diversiloba Spruce of my MSS. At the time of preparing my MSS. I had not seen Spruce's paper "On Lejeunea Holtii" and was misled by his statement in Hepat. Amazon & Andes respecting L. lucens as a South American plant.

What I took on examination by hand lens alone to be Anthelia julacea proves to be a slender

form of Gymnomitrium concinnatum

As regards making an extra plate for the Manual my feeling is as follows.

1. I am no draughtsman. Any work that I could do would be bungling by the side of the elegant work of Sullivant or even that of Sprague.

2. I have no spare money to pay a competent artist and little time to spare to select and prepare the necessary tracings for the drawings.

3. If however the publishers wish to pay the expense of preparing the necessary drawings I will make a selection and either furnish copies of the plates or tracings from which the draughtsman can work. I could not possibly get them ready before the end of October.

Yours truly

L. M. Underwood.

[illegible] As I have gone through your
MSS. I have found that it needs a
complete revision, and the necess-
ity for omissions & additions
I have more & times regretted here
indeed not to read it in the whole.
At, even as it is it is so terribly a
copy of your ~~[illegible]~~ Catalogue, and I have
found that it could in various ways, be
reduced in compass with remarkably little
loss in any chapter & often no one whatever
and to its decided improvement, in my
opinion. In this way it would come within
30 - 35 pages I think. ~~[illegible]~~
I have been loath to
set with this copy to your instructions,
but the alterations ~~[illegible]~~
would have to be subjected to your
approval in proofs & modified
them to follow you. One of the more
serious objections that I foresee is the adop-
tion of Austin's description of Anthocoris
(including Acompocoris).
The only species, I think, the notes enclosed
has access more to Anthocoris than any
in the description, but the species is evidently
in single & the detail therefore has more weight.
as mistakenly given by Austin

COLUMBIA COLLEGE HERBARIUM.

NEW YORK, Oct. 5th 1889.

Dear Dr. Watson: —

Many thanks for your full answer to my inquiries about Lygodium Kotschyanum. I will let you know and send specimens when I learn a little more about it. —

Enclosed are three packets some Norway duplicates - mostly weeds. — Ne. Richardsoni may be acceptable. It is the first we have in the collection. — Yours sincerely

Eli. Britton.

HERBARIUM OF HARVARD UNIVERSITY,
BOTANIC GARDEN, CAMBRIDGE, MASS.

or 2005² (12)

Diplophyllum albicans – Hook. Jung. t. 25

Lunularia vulgaris Bisch. t. 6) Och. 1884

Dear Prof. Underwood,

I find excellent figures of the following unpresented genera for the new plates –

Jubula Hutchinsiae – Hook. Jung. t. 1

Blepharostoma trich.ᵐ – Fl. Dan. t. 2513²

Liochlaena lanc.ᵃ – " t. 2940²

Jungerm.ᵃ Schraderi – " t. 2957'

~~pumila – " t. 2144~~

barbata – " t. 2123

~~alpestris – " t. 2957²~~

~~[illegible] – " t. 2005'~~

Helleriana – " Suppl. t. 100²

inflata – " t. 1945²

Nardia crenulata – " t. 1773'

There remains unillustrated

Blyttia Taylori – (Hook. Jung. t. 57)

~~Diplolæna~~ (Cincinnulia) pusilla Bisch. t. 6

It [illegible]

these, & will select from under Jungermannia above those species that will best illustrate the different sections, or suggest others ones, I will try & get the drawings made here. I omit, Lunularia, as merely an introduced plant, & Diplophyllum, as not yet found in our territory.

Do you object to the use of persistent in place of inner involucre? I see that Spruce uses it, & it is much less cumbersome.

I would write Calypogeia multiflora, Hook. That Spruce corrected Hooker's description is hardly sufficient reason for citing him as authority for the species. This is a generally accepted principle.

Porella Deutata is the oldest name for the species under the genus. Lindberg should rather have taken the specific name sinuosa, but he did not do it, and it is only making synonymy to do it now. Yours truly

It is scarcely worth while to go into a discussion of points of nomenclature. Specific names as such begin with linnaeus. Binomial phrases are to be found before his time occasionally but they are accidental and not systematic as names in the linnaean sense. So Tournefort is considered the author of the genus, though many generic names have an earlier origin, but the same principle that would deny to Tournefort would scarcely find a resting place till it reached Julius Caesar and Tournefort. Jungermannia, in fact, as Rupp has gave no characters by which it could be distinguished as a genus and all his species are now placed in other genera. Michelli defined it as a genus of which only one does seem to properly attach to the author of the present Jungermannia and linnaeus credited it to him. Both he and Rupp wrote the word with the double n, of doubtless knew as well as Jungermann himself how his name should be written.

Lindberg & Spruce are certainly good authorities. Still I like to think of just for myself.

Your kind enquiries over the Botany of California. Those sheets were collected and presented so long ago. I

Southington Ct Oct 11/89

Prof Eaton

Dear Sir

Prof Eaton informs me that you are preparing the new edition of Gray's Manual. will you kindly inform the publishers that I desire a copy as soon as out.

I would also like to inquire if you have included the new Hawkweed Hieracium aurantiacum. it has recently appeared in this place in some abundance and I have included it in my "Flora of Southington Ct"

Resp'y Lyman Andrews

Southington Ct

Syracuse, N.Y.
Oct. 16. 1889.

My dear Dr. Watson

Your letter came to night and I hasten to answer it.

Your suggestions with reference to the the plate are excellent and I suggest the following:—

(1) Jubula as cited altho our ~~form~~ differs somewhat from the type as known in Europe.

(2) Blepharostoma as cited or Hook. Brit. Jung. t. 7

(3) Liochlaena as cited

(4) Mylia Hook Brit. Jung. t. 57.

(5) Nardia as cited

(6-9) Jungermania as cited

I would suggest if more than one form is illustrated (which is very desirable) I should have 1) Schraderi (2) barbata or ventricosa (3) Hilleriana (4) inflata. (over)

10. Diplophyllum albicans: Hook. Brit. Jung. t. 25. * Cf. Gottsche. Hep. Amazon p. 417 note for generic reference.

11. Grimaldia rupestris. Cf. Bischoff in Acta. Acad. Caes. Leop. Carol. XVII, Tab. LXVIII. where a most excellent illustration occurs. Sullivant drew mostly from this work for his illustrations of Marchantiaceae

I see no good reason for excluding Lunularia since a Manual is for the determination of species whether native or otherwise. You will find excellent illustrations in Bischoff's work. This would make 12 species illustrated which is the number on the other three plates.

In regard to the remaining questions I will answer them seriatim.

1. I do not like either expression "perianth" or "inner involucre". I used the latter in my "Catalog" following a verbal suggestion of Dr. Gray given me in 1883. I feel that

* You are mistaken with reference to Diplophyllum not occurring in our region. It is very abundant in the White Mts and I have it from R.I. Washington D.C., Virginia and southward to N. Georgia. I have it so recorded in the MSS.

a new terminology ought to be adopted that will accord, as far as structures are homologous, with that of the mosses. I am not particular which you use and I am not quite ready to propose any other term. "Perianth" is the most common of the four terms already used to express this structure.

2. It must be Cephalozia multiflora Spruce! Lindberg redescribed a rare species which had been long known as Jung. connivens Dicks. English botanists had for years mistaken the true multiflora calling it connivens. Lindberg therefore described the true connivens as multiflora.

Spruce (Cf. "On Cephalozia p. 37. also note p. 39) did not simply correct Lindberg's description but on the contrary described a totally different plant!

There are therefore these two species

1. Cephalozia multiflora Spruce Europe, America
2. C. connivens (Dicks.) = C. multiflora Lindb. Europe.

(over)

It might have been wiser if Farmer in his remaining had chosen some other name than multiflora Had he named it C. dicksoni n.sp. it would certainly stand to his credit; he did name it C. multiflora n.sp. which does not alter the case so long as it is a distinct plant from C. multiflora Lindb.

3. Regarding the next case I am not quite so clear. It is easy to see what is right; I am less certain as to usage. The facts seem to be in this case

1. Hartmann described the plant as a variety in 1832 as Jungermania platyphylla β dentata
2. Nees described it as a new species in 1838 under the name Madotheca rivularis.
3. Lindberg (1869) while recognizing M. rivularis as a synonym erroneously gave it the name Porella dentata. Had Lindberg described the plant as Porella Madotheca dentata

or under any other name, that name would have yielded to Nees' name without question. I see no difference where a change of genus is involved. It was he who recognized it as a species; it is but justice to him to call the species Porella rivularis (Nees). Had Lindberg called the species Porella Hartmanii n. sp., in accord with the doctrine of "the first name under a genus", that name would, of itself, have priority over rivularis. That would be a mere quibble, it would not be right. As to increasing the synonymy, all European hepatics that have been known 40 years have so large a synonymy that a small increase would not be perceptible. If the plant is a good species its name in the future will be whatever we call it in the Manual. as I have written it, if Lindberg's error must be corrected, sometime, it is not too soon to commence.

Yours very truly,

Lucien M. Underwood

I will send an addition to Porsombronia tomentosa.

Syracuse N.Y.
16 Oct, 1889.

Dear Dr. Watson,

Your two letters are at hand and I will reply to some of this questions at length.

I wish to say, first of all, that I am supposed to be responsible for whatever may appear on the Hepatics in the "Manual"; that, following the custom in former editions, I understand that I will be credited as the author of that portion of the volume. This being so I feel that it is for me to have the decision of the subject matter, yielding in matters of form for sake of uniformity with the rest of the volume. With this understanding of the matter, I yielded without question the matter of arrangement of keys in which I was perforce obliged to sacrifice clearness and simplicity; I yielded also in

in the matter which you denominated the "double headed nomenclature" i.e. double reference of specific names, altho in my judgment that involves an ethical question also. In matters, however, that involve the subject-matter including names of species I feel that I have the right to decide.

Now as to the Porella matter
You say Lindberg thought "it was right to take the name dentata". I have before me a copy of Lindberg's "Utredning af Skandinaviens Porella-Former" where (p. 342) Porella dentata (Hartm.) Lindb. is first christened. I see nothing to prevent me from believing that it was "scientific expediency" that led him to use this name!

But laying aside jest, I maintain my position, as in accord with

the usage in zoology and cryptogamic botany quite generally, to reject the name Porella dentata and write the name Porella rivularis (Nees)! This will be my usage in the matter in my writings elsewhere in the future as it has been in the past (Cf. Bot. Gazette, XIV, 195). This being the principle on which cryptogamic botanists are working it will be the usage of the coming generation. Now is a better time to make an inevitable change (i.e. on the first public statement in America), than later after a certain familiarity with the other form has been acquired here.

As for examples of this practice outside of cryptogamic botany I have only to refer to one of your own masterpieces.—Botany of California I, 229 where I read

"9 [odetia] purpurea Watson" in place of the earlier and "first name under the genus" Godetia Willdenowiana Spach. and other examples of the same character elsewhere in the volumes. There will be other notable examples of the same thing in the Manual under the Gamopetalae unless Dr. Gray's nomenclature of the Synop. Flora is widely departed from.

I have given great care to the nomenclature in my MSS. and if I have made errors, it will be from the fact that I have limited means for supplying myself with needed literature. This lack no one can appreciate more than I, myself.

I append a note on Kantia and one on the reference to Jungermannia to its original source.

5)

(over)

Raddi published (1818).

Calyptogeja

A. Exampligastriatae.

C. ericetorum.

C. flagellifera.

B. Ampligastriatae.

C. fissa. [= K. trichomanis]

Gray published (1821).

Kantius

K. trichomanis.

Dumortier published (1822).

Cincinnulus.

C. trichomanis.

The Gazette articles showed Raddi. Calyptogeja Gr. = Kantius = Cincinnulus Dumortier

There are several other errors in that article for which I am not responsible having had no proof.

There are in the above, two good genera which are the equivalents of Raddi's two sections The first properly takes the name Calyptogeia Raddi.

The second just as properly takes the name of Kantia S.P. Gray. Both have many spec

In regard to the matter of Jungermania I prefer to follow Dr. Spruce and Lindberg, whose authority as hepaticologists will not be called in question, in using a single n. It would be a novel proceeding to write it "Jungermania Micheli" as Micheli merely adopted Rupp's name Dr. Spruce writes "Jungermania Rupp. L." to which I would not object seriously yet I see no reason why the name of every one making emendations to a genus should be tacked on. I could not consent to the use of "Jungermania Micheli" I have neither the works of Micheli nor Ruppius in my library but Lindberg's abstract from from both works in his "Hepaticologicus Utveckling" gives the

7)

16 OCT 1889

orthography with one n. So also in his quotations of ~~Ludberg~~ Linnæus' earlier writings.

I see by consulting Linnæus' Systema in the College library that there uses the form Jungermannia.

Yours respectfully
L. M. Underwood

Subfamily Acmeae
Cabomyaceae - Kantia

This article goes in under your name certainly, and then I can also refer to it in the ~~other~~ chapter of the

HERBARIUM OF HARVARD UNIVERSITY,
BOTANIC GARDEN, CAMBRIDGE, MASS.

Oct. 19th 1889

Dear Prof. Underwood,

I am sorry that this difficulty about nomenclature has arisen unexpectedly to alter this shape of the work. There has been nothing of the kind with the other collaborators in the revision. There must of course be uniformity throughout the book in respect to nomenclature as to other things, and as it is still "Gray's Botany" I am the more obliged to make it strictly conform to what I know would be the principles upon which Dr Gray himself would have in

the revisions had been kindly, & it is my endeavour also to keep it up to its former high standing in every respect. Nothing consequently has yet been added to my criticisms & been more or less modified as seemed to me necessary, & with scarcely an exception my alterations have been approved & adopted by the contributors.

The same reasonable course of procedure must apply also to the Hebraica. The only alternative that I see is to omit them altogether, notwithstanding that much of the matter is already in type. I have read as yet only the first of the proofs which I send herewith

Syracuse N.Y. 25 Oct.

Dear Dr. Watson,

Had I known at the beginning that my MSS. was to be rewritten without my knowledge I should certainly never have furnished it for that purpose. And if I should consult purely personal reasons now I should say, send the MSS back. Two reasons, however, suggest themselves to me with that force that I dislike exceedingly to do this. These are (1) the fact that many botanists (judging from my correspondence) would feel a disappointment were the hepaticae not to appear in the Manual, and (2) the more important fact that ~~this~~ the stimulation of the study of local distribution and life of these plants throughout the country could come by no other means as well as thro the medium of this publication. That this stimulus is desirable no one feels more keenly than myself and it was for this end that I undertook the matter in the beginning.

But to turn to your revision. That you have accomplished "Compression" the work clearly shows. But this is accom-

plished (1) By cutting off details that are often essential to the determination of a species or if not essential strongly desirable, (especially since it is highly probable & nay almost certain, that many new forms will be discovered in this same manual region that with incomplete descriptions would ~~result~~ be likely to be confused under species herein described. (2) By sacrificing clearness of statement and substituting therefor statements not only unintelligible but rhetorically wrong. (3) By introducing statements that do not represent facts.

To illustrate: Take the first line that appears under Hepaticæ (Prof. p 257a). You corrected my form of expression in the first sentence by omitting two words "consisting" and "a" which I have supplied in the proof, both of which are essential to make the sentence grammatically correct. In the next sentence with was changed to of, and there appears the barbarism "leaves of ---- no midrib" A few lines lower down sporogonium is made a synonym of capsule which is a botanical error. A little below flagella

(the Latin plural of flagellum, i.) is corrected(!) to flagellae!

Still below appear the words "Dyoecious", etc. which to me appear in type for the first time in the writings of any professed bryologist, in place of "dyoicous", "paroicous" and "autoicous" as used by bryologists universally. Has it come to this that we must "think for ourselves" in order to oppose the best usage of such men as Schimper, Lorentz, Stephani Braithwaite and Lindberg?

Two lines below appear the expressions "overlapping upwards" and "overlapping downwards" the last of which would cause the most stolid hepaticologist to smile!

But I need not multiply instances since they appear so noticeably on the first page of proof. I am willing to bear the responsibility for my own blunders but I will not submit to the burden of responsibility for those of others.

Now I am willing to do the reasonable thing in the matter; but feeling that as I have probably more familiarity with

the plants in question, and knowing from twelve years of use of the Manual in the class room how students will stumble over expressions that to one perfectly familiar with the structures described might seem perfectly clear, I feel that certain things must be changed. As compromise is the rule I am willing to keep the descriptions for the most part of the same length as you have cut them, but where they are wrong in fact and misleading in statement I shall correct them.

I will allow the two n's in Jungermannia (only filling a bill of exceptions) and am willing to let Porella denticulata take its necessary corrections later.

I cannot approve synoecious, etc. If you are unwilling to allow synoicous etc they must all be cut out and "monoecious" substituting, (and allowed for uniformity's sake) this monoicous has the better usage among bryologists at least. My preference in this matter would be to use for the hepatics "monoicous", "dioicous" and for the former, to be more specific in statement the terms "synoicous", etc. If this cannot be allowed my next choice is stated above.

5)

I append a list of places where this must be done in case my preference is not allowed.

Since the above was written the rest of the proof (i.e. beyond Frullania Oakesiana) has come, and I have gone over it carefully. I have added no more that I felt was absolutely necessary and in certain of the larger genera (E.g. Cephalozia & Lophozia) I feel that so much has been cut out in your revision that a novice will be sadly crippled in attempting to use the Manual for the identification. It is unreasonable to apply phanerogamic methods to the determination of microscopic cryptogams; slight differences of expression are here of more importance.

Some further notes of inaccuracies in your revision.

1) Lejeunia should be Lejeunea

2) Under L. lucens you say "under leaves ½ as large as the upper" — "Upper" leaves on a hepatic would demand a new genus!

3) Under the § Thyopsiella in Frullania you have introduced

6)
a statement which is only partly true of the S and even misrepresents it. While true of our species of the section all species of Hygrocrella do not have moniliform cells.

4) Is "antheridial spike" a case of compression or even increase of clearness? I wrote "male spike" which is sanctioned by the best usage.

5) Cephalozia multiflora Spruce, as forms with us is not C. multiflora Lindb. neither is J. connivens Dicks. forms in America to my knowledge! I have clearly explained this whole matter once before to you. Cf. my former letter and then let it stand as I have corrected it in the proof.

6). Your use of lobes when I used lacinia or divisions is incorrect and inapplicable in the instances where I have made the correction.

7. Scapania undulata Dumort. is right as I wrote it in my mss.

The synonymy of this is as below.

7)

Jung. undulata L. Spec. 1598

Jung. undulata Nees Europaische Lebermoose I, 184 (1833)

Scapania undulata Dumort. Recueil, p. 14 (1835)!

Plagioclila undulata M. & Nees. Europaische Lebermoose. III, 520 (1838)

Scapania undulata M. & Nees. Synopsis hepaticarum. p. 65 (1844-7)

Dumort has clearly priority by 9 years at least. The fact that you may not have seen his "Recueil" is no excuse for correcting my copy.

S. subalpina is also one of Dumort's species.

7) You have omitted (Eu.) from my copy in several places.

I think Harpanthus Flotowianus ought to go in as it will without doubt be found in our region, and especially since it is figured in the plates

I wish I had more faith that your corrections to my MSS were for the best. Many things make me skeptical on this point.

Yours truly L. M. Underwood.

Syracuse, N.Y.
30 Oct 1889

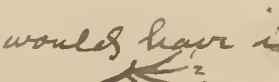
Dr Watson

Dear Sir:-

I return the balance of the proof. As in the other case I have changed it just as little as possible. In three cases you had left out portions of the descriptions that were diagnostic and I have been compelled to put them back.

In one place I had written "flexuous by repeated innovation below the summit" and you changed it to "flexuous, with several offshoots below the summit". This shows how one unacquainted with the structure of the plant in question is liable to make blunders.

The structure of the stem is this

You would have it 

In regard to Preissia I believe in P. hemisphaerica
If, however, you use "the first name under
the genus" why pass over P. Italica Corda
Opiz. Natural. 647 (1829) when P commutata
Nees was not written until 1838.[2] If it
is not changed to hemisphaerica leave it
commutata.

If you will not accept Lunularia
vulgaris Micheli you must certainly
recognize Lunularia vulgaris Raddi
Opusc. Sc. di. Bol. II, 355 (1818) four years
older than L. cruciata Dumort. The former
name (L. vulgaris) has moreover the authority
of long usage by Nees, Bischoff, Gottsche
and others.

Likewise in Sphaerocarpus.

If S. terrestris Micheli wont suit you
just yield, please, to the inexorable law of
"the first name under the genus" and write
the first Linnaean Sphaerocarpus terrestris
Sm. Engl. Bot. n. 297, which antedates

by many years Bellardi's "S. Michelii" and is moreover the accepted name among hepaticologists to-day

Marsupella adusta Spruce Rev. Bryologique VIII, no 3 of "Underw." Your system of nomenclature and recognizing authorities is in my judgment a false and egotistic one. False in not recognizing the one who names the species and egotistic in making the writer responsible for species that he never named. Cephalozia flexiceps, Porella Sullivantii Kantia Sullivantii and Bazzania deflexa are not my species and to call them mine is an injustice to Austin and others who did name them.

I again repeat that Harpanthus Flotowianus showed go in!

Yours respectfully

L. M. Underwood.

Syracuse University.

Syracuse, N. Y. 4 Feb 1890.

My dear Dr. Watson

Please accept my sincere thanks for the copy of the Manual sent. It has many improvements over the last edition in style and arrangement to say nothing of the revision of nomenclature in many parts. The expression "certainly extralimital" after Harpanthus Flotovianus on p. 718 hardly expresses my belief in the matter since I have a form from near Syracuse that I cannot separate from that species and Mr. Macoun has sent it to me recently from Canada.

Botanists throughout the country owe you and Prof. Coulter a debt of gratitude for this revision

Permit me also to thank you for your kindness in sending Fournier's description. I think I have his species and that it represents a valid one in spite of his meagre description.

Yours very truly

Lucien M. Underwood

Syracuse, N.Y.
9 May 1890.

Dear Dr. Watson.

Please accept my sincere thanks for your Contribution XVI which I was not aware had been published. I enclose the .50 cents for the sheet of Vol. I J.G. I regret that I am unable to obtain a full copy.

While in Columbia College recently I saw a framed photograph of a corner in the Cambridge herbarium with you seated at a table. May I ask if these photos are on sale and if so of whom may I get a copy.

There is also a species of Diplophyllum which I would like to see. It was (I think) described by Taylor as Jungermania argentea possibly as Sphen. argentea from N.W.

America. Spruce has reported it to Diplophyllum and I am anxious to see it.

There is also another Californian Hepatic I would like to see.

Cryptomitrium tenerum Aust.

Marchantia tenera Hook

Duvalia tenera Goltsche

I did not find it ~~omen~~ in the general collection under Cryptomitrium but neglected to look under its two synonyms. It is reported in the "MSS. of Botany of California as collected by "Parry, Bigelow, Bolander, Torrey, Male inflorescence and pistillidia not seen" I am hoping to see a specimen of it. Could find none in the Torrey Herb.

Thanking you for your kindness

I am Yours very truly

L. M. Underwood.

Syracuse, N.Y.
22 May 1890

My dear Dr. Watson

I return the drawing and notes. It is sufficient to assure me that I have the same plant that is here described. The fungi (Zoopsis) argentea is a species of Cephalozia which the plant I am after is a Diplophyllum

I regret to have given you so much trouble. I am hoping now to have a year's leave of absence next year in which case I shall spend considerable of my time at the Herbarium.

Thanking you for your kindness I am
Yours very truly
Lucien M. Underwood.

Greencastle, Ind.
4 Sept. 1891.

Dr. Watson

Dear Sir.

Thanks for your trouble. I regret to have lost your last contribution as there is so far as I know no "Dr Pauw" in Indiana and the university bearing that name is at Greencastle which will be my address this year. I am trying to keep a file of all your papers. After 7 yrs of watching catalogs I succeeded this summer in picking up your second contribution which completes the series up to 1890 inclusive. I shall be glad to pay for a

copy of your last Contribution
if you can spare one.

Yours very truly
L. M. Underwood

Greencastle, Ind.
4 Oct 1891

Dear Sir:

I take this first opportunity to report on your Hepatics which were duly received. One or two I will report on later. I hope you were successful in finding Cypripedium arietinum.

I will put up a package of Hepatics for you as soon as I can get a little time.

Yours in haste
L. M. Underwood

Thanks for Obtusilobum
Retusum most of the B. malmeanum
folium. Do you find B. simplex
in Maine?

Syracuse N.Y. 21 Apr.

Dr. Sereno Watson,

Dear Sir:-

I have of Torrey and Gray's Flora of N.A. what I take to be the second volume (pp 1-504 Caprifoliaceae to Compositae) without title page or Index. Is there any possibility of getting the rest of the work? And is the ~~first~~ second half of Vol I complete in itself? Is there a title page and Index to Vol II and can I get part II of Vol I separate? If so please send it unless it is possible to get Vol I complete. I desire also Dr. Gray's contributions as follows:-

Proc. Amer. Acad. V, 114-146; 146-152; 155-173 3/4 174-190; 190-191

V, 519-556. This will complete my series of his "Contributions" since 1860.

Syracuse, N.Y. Jan 23

Dr. Sereno Watson

Mr. Pringle has send me a Marsilia from Mexico which I take to be M. minuta Fournier of which I have no description.

May I trouble You to send me a diagnosis of it. Bull. Soc. Bot. France 1860: 6-329.

Will you also give title of the article in which it is published.

Do you know Mons. E Fournier's address or is he not living?

Yours Truly

L. M. Underwood

√

Manual notes.

Mr. C. S. Raddin of Chicago Ill reports

√ Grindelia squarrosa; in an uncultivated field, Evanston, Ill.

√ Tragopogon pratense; by a roadside. Evanston, Ill.

√ Echium vulgare is very common and is increasing on our College campus. It first appeared here about 1880.

√ Lactuca scariola is rapidly spreading here. There is nearly a quarter of an acre of it in the field across from my house seeded from a dozen plants or so of two years ago at which time I send you specimens. I saw it in large patches along the railroads on my trips. — In Illinois, In Minnesota and in California.

L. M. Underwood
Syracuse, N.Y.

Cambridge, May 2. 1878

Prof. Asa Gray

To JOHN WILSON & SON, Dr.

Book and Job Printers.

1877
March 7 To compos. & electro. 116 pp. Flora of North America.

34×57 = 1938 Long Primer	27 pp.	= 52,326			
42½×71 = 3018 Brevier	83 "	= 250,494			
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JOHN WILSON. CHAS. E. WENTWORTH.

University Press,

Cambridge, August 13 1884.

Prof. Asa Gray

To JOHN WILSON & SON, Dr.

Stereotypers, Electrotypers, and Printers.

(All claims should be made within five days.)

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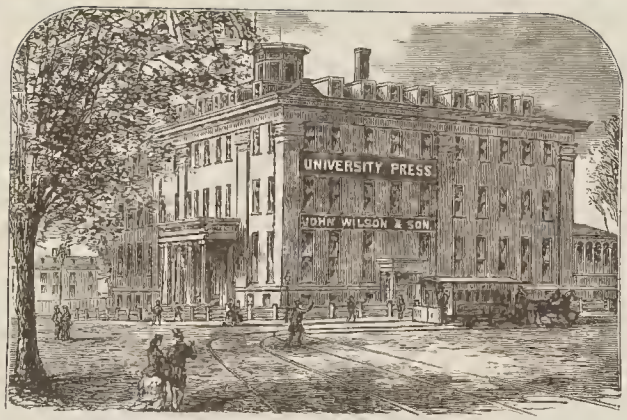

JOHN WILSON. CHAS. E. WENTWORTH.

University Press,

Cambridge, August 13 1884

Prof. Asa Gray.

To JOHN WILSON & SON, Dr.

Stereotypers, Electrotypers, and Printers.

Synoptical Flora of North America.

Volume 1. Part 2.

1-IV and 1-474 = 478 pp

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42½×72	3060	Brevier	323¾ "	989.145	
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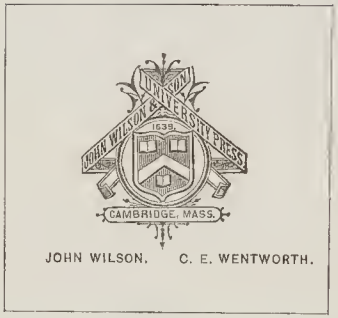

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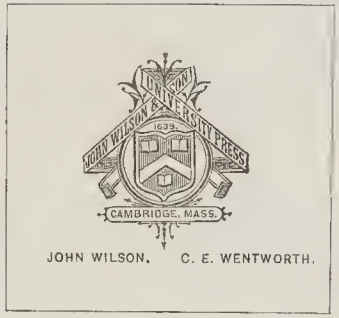

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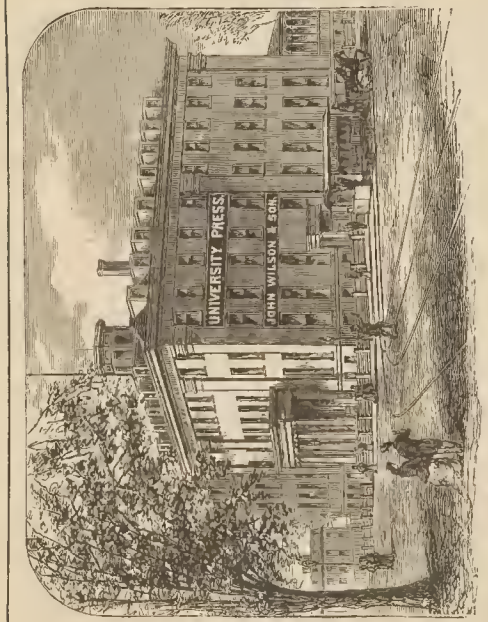

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JOHN WILSON. C. E. WENTWORTH.

University Press,

Cambridge, Mass., Dec. 9 1887.

Professor Watson.

Dear Sir:-

Messrs. Ginn, Blakeman, & Co. write us that they wish us to begin the shipping of the working plates of Gray's Botany, at once. To facilitate their purpose we send you an installment of both sets of plate proofs. Kindly return them as when revised, and advise us if they are ready to ship.

Yours truly,

John Wilson and Son

W. H. Reid

Berlin, botanischer Garten
Potsdamerstrasse 75
19/III 1883.

Hochverehrter Herr Professor,

Schon lange hatte ich mich danach gesehnt, Ihnen einige interessantere Produkte meiner Studien als Zeichen meiner aufrichtigsten Verehrung zu senden, allein dieselben bezogen sich entweder auf die Local flora (Standortsverzeichnisse) oder waren in der grossen Flora Brasiliensis verstreut oder - es gab überhaupt keine Separatabdrücke. Jetzt bin ich in der angenehmen Lage, Ihnen eine Monographie übersenden zu können, welche wenigstens in eine Art

Das Gebiet der vereinigten Staaten Nord-
amerikas berührt, ich habe es nicht an
Mühe und Geduld fehlen lassen - das darf
ich getrost versichern -, um Liselle möglichst
auf der heutigen Stand der Wissenschaft
zu erhalten. Ich bitte deshalb für Liselle
sowie für eine andere biologisch-morpholo-
gische Arbeit um eine wohlwollende Auf-
nahme. Damit Sie sich aber, hochverehrter
Herr Professor, von meiner Persönlichkeit
sogleich ein correctes Bild machen können,
habe ich mir erlaubt, meine Photographie
beizufügen; sie würden mich ausserordentlich
glücklich machen, wenn Sie mir auch Ihre
Ihrige übersenden wollten.

Genehmigen Sie den Ausdruck meiner
vorzüglichsten Hochachtung, womit ich
verbleibe

Ihr ganz ergebenster

Dr. J. Urban

Custos d. Königl. botan. Garten

Hochgeehrter Herr Professor,

Nach Absolvirung der Turneraceen habe
ich die monographische Bearbeitung der
Loasaceen in Angriff genommen, eine Fa-
milie, welche, mit Ausnahme der nordameri-
kanischen Arten, ebenfalls seit langer Zeit
vernachlässigt worden ist. Die Schwierigkeiten,
welche sich einer gründlichen Durcharbeitung
entgegenstellen, sind ganz erhebliche, bei Loasa
sind doch Aufklärungen in inneren Blüthentheile
zu reconstruiren und das öftere Fehlen der Früchte
bei Mentzelia, soweit ich bis jetzt urtheilen
kann, die geringen Art-Unterschiede.
Zur meiner Turneraceen-Arbeit wurden

Sie ersehen haben, dass ich es mit einer solchen Bearbeitung sehr ernst nehme, dass aber auch die Resultate absolut ganz befriedigende sind nicht blos auf rein systematischem, sondern auch morphologischem und biologischem Gebiete. In letzterer Beziehung besonders suche ich in nächster Zeit zu publiciren, was ich irgend erreichen kann. Frische Turnieren war dort sehr wenig; in wenigen Tagen habe ich schon mehreres und hoffentlich sehr Material zum Säen zu erhalten. Schmidt wird auch Ihren freundlichen Auftrag ausführen und Sie ganz sicher bald hören. Das botanische Material, welches in das botanische Institut gelangen wird, kann ich Ihnen gleichfalls zur Verfügung stellen. Ihre Sammlungen sind angekommen und gelegentlich auf reife Samen durchsehen und lasse auch Ihnen die Ergebnisse der Untersuchung zukommen. So weit ich Ihnen die Herbarien in Cambridge benutze, so hoffe ich doch den Schaden durch etwaige Ergebnisse meiner Studien am lebenden Materiale wieder aufzuwiegen.

Indem ich mich Ihnen bestens empfehle, verbleibe ich

Hochachtungsvoll und ergebenst

Dr. J. Urban.

Berlin, Bot. Garten
Potsdamerstrasse 75
17/XII 1883.

Berlin, botan. Garten
Potsdamerstrasse 75
16 Januar 1886

Hochgeehrter Herr Professor,

Sie haben vielleicht von den Gebrüdern Arwel und Adolph Krause gehört, welche in den Jahren 1882 Alaska bereisten. In gemeinsamer Veranlassung sammelten sie auch Samen für den hiesigen botanischen Garten, aus welchen manche interessante Pflanzen hervorgingen. Die Bestimmung zweier Leguminosen ist mir aber nicht gelungen, theils am Mangel an übereinstimmendem Herbarmaterial, theils an Zeitmangel. Da möchte ich Sie nun freundlichst bitten, mir die Namen mitzutheilen, wenn Ihnen solches nicht schwer fällt.
Der von hier nach Puerto-Rico gereiste Herr Sintenis ist jetzt seit 3/4 Jahren dort ausserst fleissig mit der Durchforschung der

Flora beschäftigt; ich werde mich bis dann
Herbst-liche Arbeit dort lassen, so dass ich mich
Vegetation zu sehr gründlich erforschen kann mit
5 mächtige Kisten sind bereits eingetroffen, die
3 erste Lieferung besteht, so dass die Vertheilung
der Pflanzen an die Abonnenten bereits statt
finden. Die Kisten einer Anzahl besonders
schwieriger Arten (Lamiaceen, Myrtaceen, Cyph.
Diag) muss ich jedoch bis zum Abschluss der Expedi
tion zurückhalten, ebenso die Beschreibung mehrerer
Novae, die ich erst nach Vergleichung meiner
und de Candolle vorliegenden mir, die Kommel
Arten erst zu letzt zur Ausgabe. Die Publikation
erfolgt in Acta Hort. Petr. Ich arbeite
in Comm. et Herbar. pet. nov. et minus cognit.
In der Zwischenzeit von jetzt bis zur Rückkehr
des Herrn Fiedler wie ich nun meine Loasaceen
Monographie beendigen. Und da komme ich nun
noch mit einer Bitte: Niemand hat Syngenesia
Aequalia aus Ecuador als Sie. Da ich nun in jeder Sendung
einen Vertreter jeder Section eine Analyse gebe, so

erlaube ich mir die ergebene Anfrage, ob Sie mir
von der Pflanze nicht eine Blüthe und Frucht
reg. skizzen zur Untersuchung und Zeichnung
zurückliefern könnten, ich werde sie sehr sorgsam
behandeln und Ihnen sofort remittiren.

Mit vorzüglicher Hochachtung zeichne ich

Ihr ergebenster

I. Urban

Custos des kgl. botan. Gartens.

Schöneberg bei Berlin
8 Nov. 1886.

Hochgeehrter Herr Professor,

Ich muss sehr um Entschuldigung bitten, dass ich Ihnen erst so spät die Separatabzüge zurückschicke, es hat das hauptsächlich darin seinen Grund, dass der Bau einer Villa mich ganz gegen meine Erwartung von meinen Studien abzog (ich konnte nur in den Vormittagsstunden wissenschaftlich arbeiten), die übrige Zeit nimmt die bureaukratische Thätigkeit und Sorgen in Anspruch). Diese Klebst rate ich die Correcturen meiner Studien wieder auf - Allein ich konnte mich mit der Separatabzüge nicht so bald trennen, weil ich es noch gewünscht hätte bei jeder anderen Sendung alles gethan hätte und wie oft es auch hier vorkommt, der Grund dass [illegible] in dem eigenthümlichen Band der Verhältnisse: ich muss gestehen: trotzdem Arbeit in meinem Leben hier mein wissenschaftliches Verlangen auf, fortwährend ist mir die grösste Lücke gegeben habe, nach einem Abschlusse auch die übrigen Sammlungen zu ordnen. Es bleibt nur das Resultat, welches sie

schon fanden, dass alle übrigen Charaktere auf Eisenoxide
als nächste Verwandte hinwiesen. Ich spreche die Hoffnung
aus, dass es Ihnen und Ihren Mitarbeitern gelingen möge,
noch einige Arten oder Lösungen aufzufinden, welche
auch in grösseren Uebergangsbildungen Darbieten.

Mit Ihrem Professor Eichler geht es ja in der letzten
Woche ein wenig besser. Leider muss man die Besserung
auf eine ganz äusserliche Ursache zurückführen, und zwar
auf den Trost, den ihm der behandelnde Professor auf den
Kopf hin bezüglich der Leukämie spendete, d. h. dass
seinem Leiden Ihrem Herrn Arzt von Breslau her für
Prof. Eichler Kommen lassen. Er selbst hatte sonst voll
ständig für sein Leben abgeschlossen und sie erklärt, dass
Alle sein letzter Winter sei; der Tröstliche ist, dass wir
Alle dies Ueberzeugung theilen müssen. Es ist ebenso
Entsetzliches, so Manche lang dem sicheren Tode entgegen
sehen zu müssen — Mit den Seinigen ist Gustav, eine
unversorgte Familie (6 Kinder von 1½–14 Jahren) nebst
Frau und Schwiegermutter zurück zu lassen.

Ich habe jetzt bei der Akademie der Wissenschaften
eine Eingabe gemacht um eine Unterstützung für die
botan. Erforschung des Riesengebirge für Herrn
Herrn Baron Eggers, welcher dieser Sommer bei mir
war, habe ich für die Durchführung der Untersuchungen ge-
worben. Da Prof. Eichler leider für keine Courage für solche
Untersuchungen hat, so wird Herr allein auf privatim
werden. Sollten Sie sonst geneigt sein, sammeln, könnte
mir nicht gelegentlich, da er so viel Geld verdient, ca.
200 Dollars pro Monat, und da er nur reisen will, wenn
er mal Lust hat und ohne eigenes Risico die nöthige
Leiden bei der Bank deponirt hat. Jetzt wird Akademie
ein Zuschuss geben, wie: Herr Conrad Krug — ich beabsichtige
mich in der Weise, dass ich jede gelieferte Centurie mit
100 Mk. bezahle. Im Uebrigen übernimmt Baron Eggers
das Risiko und die Vertheilung an die Abonnenten. So wie
das Geld von der Akademie bewilligt ist, gebe ich Ihnen
Nachricht, wenn Sie event. abonniren können.

Für heute empfehle ich mich mit den besten
Wünschen für das Wohlsein

Ihr ergebenster
J. Urban

I beg your pardon for returning the Sympetaleia so late. The main cause of this is the building of a villa, which kept me away from my studies, contrary to my expectation. I am able to do scientific work only at my leisure, the rest of my time is required for my bureaucratic occupation in the garden. This fall I took up again the Loasaceae for study. But I was unable to part with the Sympetaleia so soon, as I would have done with ~~any~~ other species or genus after close examination
The reason for this lay in the peculiar structure of the androecium. I must confess: here for the first time in my life my morphological judgement leaves me, although I have taken the greatest pains to find a connection with the remaining genera. That result remains only, which you have already found, that all the remaining characters point to Encnide as the nearest relation. I express my hopes, that you and your fellow-laborers may succeed in finding several species or genera

which shall present also in the androecium transitional structures.

Prof. Eichler has been improving during the last few weeks. But, alas! we must trace this improvment to a mere external cause, to the consolation which the most renowned professor on the field of Lew..... administers. The ministry had ordered this gentleman expressly for Prof. Eichler, from Greifswald. Prof. Eichler had already closed up for this life and explained to me that this was to be his last winter. It is sad that we all must be of the same opinion. It is terrible to look Death in the face for months, and at the same time to think of a family to be left alone and ..... not privided for, (6 children from 1½ to 14 years of age) and wife and a sister.

I have petitioned the Academy of Seiances for a support for the botanical exploration of the high mountains of St. Domingo. Mr. Baron Eggers, who visited me this summer, is gained for this enterprise. As Prof. Eichler has no courage for such enterprises, I have to do it all alone and privately. We can't employ Lintenis any longer, although he is a good collector. For he spends too much money, c.v. 200 Dollars per month, and he will not travel unless he can draw the necessary money at the bank without any risk on his part. Now the Academy is to give a decision. We, Count Krug and I, take take part in this way: we pay 100.— for every century received. Baron Eggers takes the risk and distributes to subscribers. I will advise you as soon as the appropriation is made, in order that you may be able to subscribe.

Friedenau bei Berlin
7 Dec. 1887.

Hochgeehrter Herr Professor,

Durch die Oldenbourg'sche Verlagsbuchhandlung zu München sind Ihnen sechs Exemplare von Fascikel 100 der Martini'schen Flora Brasiliensis zugegangen. Ich erlaube mir, Ihnen anbei die Rechnung zu übersenden, mit der Bitte, den Betrag entweder an mich oder direkt an die Oldenbourg'sche Verlagsbuchhandlung in München (Glückstr. 11) einsenden zu wollen.

Indem ich mich Ihnen bestens empfehle, zeichne ich Hochachtungsvoll ergebenst

Dr. J. Urban.

Remitted 160 ℳ, Feb. 25, 88 to
Dr. Urban
Ack?

Friedenau bei Berlin
2 April 1888.

Hochgeehrter Herr,

Für Ihren freundlichen Brief vom 28 Febr. sage ich Ihnen meinen verbindlichsten Dank, ebenso für die übersandten 160 Mk. - für die Abreise und Fassi 100 Mk - Martinique Flora Brasiliensis.

Das Sorghi-Tal bei mir. Alle auf die Liste erschöpfen, kein Obstmesser weder in Deutschland noch ausserhalb gewesen wie so allgemeine Liebe und Verehrung wie er. Ich habe vorigen Sommer auf die schmerzlichste bedauert, daß A. Gray an Berlin vorbeifuhr, ohne uns zu besuchen.

Ich würde für Ihre freundlichen Anerbieten auch weiterhin die Abbildungen der Flora Brasiliensis in Empfang zu nehmen, ausserdem wenn nicht die Oldenburgische Verlagsbuchhandlung bei 4 - 5 Hefte 101, welches Anfang

[illegible]
Vers
Lieb
Myr.
Colum 6

Februar ausgegeben wurde, bereits mit der Absicht
versehen in Packeten Verkauft gebracht wird und deshalb
schon die Nummern 101 und später 102 Packete
Samen hatten. Es war eine auch von obigen Herren
bestätigte Thatsache, daß die amerikanischen
Exemplare schon 8 Tage zur Vertheilung gelangten.

Das Packet Nr. 1 als Fortsetzung der Sinclair'schen
Puerto Rico Nummer 300 Nr. in 6 Packeten
einzeln verpackt in einer Kiste, welche ausserdem
900 Species in 18 Packeten für J. D. Smith
(Baltimore 505 Packete Nummer) und 350 Species
für in 6 Packeten für Wm. M. Canby in
Wilmington, Delaware enthält. Die übrige
Sendung hatte ich an Herrn J. D. Smith gerichtet
und ihn gebeten, die für Cambridge bestimmten
Packete Ihnen zukommen zu lassen.
Nochmals bitte ich Sie, so freundlich zu sein,
die Uebergabe der Packete an J. D. Smith und
an Canby zu übernehmen. Die Smith'sche Collektion
ist viel grösser ausgefallen, als ich dachte, weil
Herr Smith nicht blos die Arten, sondern auch
Standorte nimmt.

Die für die besonders 300 Nr. betragen 90 Mk.
Berechnung für Kiste und Porto werde ich das
nächste Mal beifügen, wenn wieder eine Sendung
abgeht.

Hochachtungsvoll und ergebenst
Dr. J. Urban.

Friedenau bei Berlin
21 Nov. 1888.

I thank you very much
for the very interesting and very dear list
of the writings of Dr. L. Berg, from which a young botanist can see
how much application life may contribute for the science.

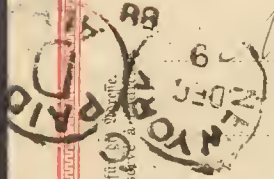
We were very sorry, last year, as you were in Germany, that you
could not come to Berlin to visit us. We all should have been very
happy to see the greatest of the North american botanists.

I am Yours very faithfully
O. Uhlworm

Deutschland. — Allemagne.

Postkarte. — Carte postale.

Weltpostverein. — Union postale universelle.



Mrs. Asa Gray

Cambridge (Mass.)

Vereinigte Staaten von Nord-Amerika.

Friedenau b. Berlin, den 9/V 1891.
Sponholzstr. 37.

Dear Sir,

I am now finishing my monograph of the Loasaceae. I have examined a great many of herbria of the European continent, but I have not found

Mentzelia cordata Kell.
" hirsutissima Watk
" arenaria Torr et Gr.

Can you lend me a good specimen of these species for my studies? You will receive it back in two months. Also I have not seen the types of M. floridana Nutt. oligosperma Nutt. and pumila Torr. et Gr. I should be very happy to have some material well verified of these species.

Naturally I should prefer to receive a specimen of these species for our botan. museum

You know that I am second director of the botanical museum and herbarium of Berlin. Therefore, if you need any material of our museum for your studies, I beg you to tell me.

In the future I hope to communicate you many duplicates of rare plants.

Yours very truly

I Urban

KÖNIGL. BOTANISCHES MUSEUM. Berlin, W.
Grunewaldstr. 6-7. 31 Aug. 1891.

Dear Sir,

We thank you very much for the parcel, which you have sent us these days, with the interesting plants of the herbarium A. Gray. and we hope that we can give you, in the next time, a good collection of rare plants.

Further I thank you also for the Contributions of North American Botany which you have had the kindness to dedicate me.

Yours very faithfully
I. Urban.